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**The Success of Job Applications:  
A New Approach to Program Evaluation**

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# **THE SUCCESS OF JOB APPLICATIONS: A NEW APPROACH TO PROGRAM EVALUATION**

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## **Abstract**

In this paper, we suggest a novel approach to program evaluation that allows identification of the causal effect of a training program on the likelihood of being invited to a job interview under weak assumptions. The idea is to measure the program-effects by pre- and post-treatment data that are very close in time for the same individual. Our approach provides useful information on both, average effects of the program as well as information on the effects of the program for each individual. Evidence on individual treatment effects is helpful as it can be used to improve the targeting of programs.

**JEL-Classification:** I38

**Keywords:** evaluation, active labour market program, correspondence testing

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## 1. Introduction

*The best solution to the evaluation problem lies in improving the quality of the data on which evaluations are conducted and not in the development of formal econometric methods to circumvent inadequate data.*  
(Heckman/Smith 1999, p. 3)

There is no doubt that in the presence of tight government budgets credible policy evaluation is very important. There is also no doubt, however, that the evaluation of policy programs involves fundamental methodological difficulties. In this paper we suggest a new approach to the evaluation of labour market programs that relies on a novel experimental design and on self-collected data that help to identify the causal program effect under rather weak assumptions.

We analyse the impact of participation in active labour market programs that are offered to unemployed individuals as part of the Swiss labour market policy. Since the second revision of the national unemployment insurance act in 1997, the unemployed are supposed to take an active part in improving their skills and job fitness. The different measures can be divided into three broad categories: training programs, public employment programs and wage subsidies.<sup>2</sup> This paper focuses on a particular training program, which is meant to improve basic computer skills. These courses last between two and three weeks and teach basic word processing or spreadsheet calculation skills. Participating individuals receive a course certificate upon completion of the course. Enrolment in these programs is comparable in size to enrolment in other training programs, such as basic job search training, language courses and other vocational training. About one fourth of total expenditure on active and passive labour market policy measures is spent for training programs.

Our procedure was as follows: First, we recruited unemployed persons who participated in the computer courses. Then we sent out applications for these people before they finished their course. A second wave of applications was sent after the participants had successfully completed the course and had received a certificate. The new applications were exactly the same as before with the only exception that this time the application also contained the course certificate. The impact of the program is measured by the firms' responses. We check whether

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<sup>2</sup> For details of the program see the evaluation studies by Gerfin and Lechner (2002) or Lalive, Van Ours, and Zweimüller (2001).

the probability to becoming invited for a job interview is different for the applications with certificate than for those without.

The method we suggest in this paper has at least three major advantages compared to the existing techniques of policy evaluation. First, the pre-post comparison allows us to net out time-invariant individual characteristics. Due to the fact that we *observe pre- and post treatment outcomes within a period of a few weeks*, we do not face the problem of varying labour market conditions that usually makes pre-/post comparisons problematic. The time proximity of treatment and control observations ensures that job chances of the individuals are not significantly different between treatment and control states.

The second major advantage of our approach is that the *application process is exogenous to the participants*. The set-up of our experiment rules out any changes in search intensity and/or wage aspirations, because the researcher controls the application process. In previous studies it has been shown that the search intensity may be influenced negatively by course participation, which in turn produces a possible confound for the evaluation of the program (Calmfors, Forslund and Hemström, 2001). The same holds for changed reservation wages or aspiration levels in general. It may well be that participating in a policy measure raises the aspirations for a new job.

The third major advantage is that our approach allows us to calculate *individual treatment effects*. The majority of the datasets that are currently used in order to evaluate active labour market programs allow estimating only the average causal effect of a program for specific groups of individuals. Because we collect information on several treated and control applications per individual it is possible to address the important question of heterogeneity in the average causal effect for *each individual* who participates in the program. Thus, it is possible to study the distribution of the effects of the program across individuals. This information is critical in thinking about the political decisions leading to the implementation or the reform of an ALMP system (Carneiro, Hansen and Heckman, 2002). Moreover, knowing both individual treatment effects and individual characteristics of the participants (like sex, age, education record, unemployment record) allows to characterize those groups of individuals for whom program participation is beneficial, neutral or even counterproductive. This is of central importance for an optimal targeting of policies (Manski 2000).

According to our results, the probability of becoming invited to a job interview is, *on average*, negative but insignificant. There are, however, *important individual differences*

between the participants. While for some subjects course participation increases the number of invitations to a job interview, the opposite holds for other participants. This suggests that in designing and implementing policy measures appropriate targeting is crucial. Moreover, we find that the negative mean impact of the program can be assessed for vacancies that require good computer skills. This result is counterintuitive on first sight. A possible explanation, however, is that the participation in a very basic computer course may actually inform the hiring firm about the *absence* rather than about the presence of profound computer skills.

In the next section we describe the procedure and the design of our study. Section 3 reviews some of the related literature. In section 4 we summarize the main methodological advantages of the presented method. Section 5 discusses the identifying assumption and compares it to such assumptions invoked in cross-section observational studies. Section 6 describes the data and contains results on the effect of computer courses on the probability of being invited to a job interview. Section 7 concludes.

## 2. Design of the Study

Our study was designed to investigate the effect of computer courses on the chances to be invited to a job interview. These computer courses are part of the active labour market policy in Switzerland. They last for about two to three weeks and teach basic computer skills, e.g., the use of operating system software and an introduction to word processing or spreadsheet calculation programs.

To recruit participants for our study we contacted various educational institutions that offer the courses and asked for the permission to recruit their course participants. We then went to the corresponding courses and informed the course participants about our study. This information included a short description of the aims of the study and all procedural details. Course participants were also informed that in case of participating in our study they were to receive a compensation of CHF 200 (~US\$ 120 or € 130).

All individuals interested in participating in the experiment were individually invited to our institute for a first appointment. They had to bring their CV, copies of diplomas, other relevant certificates and letters of recommendation, i.e., all documents that are enclosed in a typical job application in Switzerland. Together with each participant we discussed what kind of jobs he or she usually applies to. Given the individual profile we searched for all potential vacancies in various newspapers and on the Internet. After having found about 10 vacancies for an applicant we prepared his or her job applications including a cover letter, the CV and

all documents about education and previous positions. Prior to sending the application to the corresponding firms, the applicant looked through all applications and signed them. This first set of applications was sent without any computer course certificate.

When a participant had finished his or her course and had received the course certificate we prepared a second set of applications. These applications were *exactly the same* as before, with the only exception that now the just recently acquired basic computer skills were mentioned in the application letter and a copy of the relevant certificate(s) was included. Again the participant looked through the applications and signed them before they were sent to the firms. Appendix 3 displays an anonymous version of the cover letter and the course certificate of a treated application as well as the cover letter of a control application.

The participants in our study had to keep track of all firm responses. To standardize this information, participants were given a form where they had to indicate, e.g., the firm's name they had applied to, the date at which the firm reacted and whether the firm wished an interview or not. In case a participant received an invitation, it was at the participant's discretion to accept the interview or to deny it. After the participant sent us the completed form the person was paid the CHF 200. Most firms that received an application letter usually answered within a period of three weeks.<sup>3</sup>

Together with the applicant's CV and the completed forms we have the following data: (i) The firm's response (wishes an interview, wishes no interview, wants further information, no response after three weeks), (ii) individual characteristics of the applicant, (iii) the firm's job advertisement (including special requirements for applicant, like computer or language skills, blue collar or white collar job, firm's name and type of industry/sector, size of the advertisement in the newspaper, hours of work, the date we sent the application to the firm), and (iv) the date the firm contacted the applicant.

### **3. Related Literature**

Our design is similar to the so-called 'correspondence testing' method. According to the latter, matched pairs of applicants apply for a job. The two applicants differ only with respect to a treatment variable like sex or race. The researcher checks, whether the invitations of the two persons differ and takes this as evidence for a treatment effect. These studies have been conducted to study discrimination issues, such as racial discrimination (e.g., Firth 1981, Riach

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<sup>3</sup> In case it took longer we told the participant to stop waiting and to send us the completed form without those firms.

and Rich 1991), sex discrimination (e.g., Neumark 1996, Riach and Rich 1995, Weichselbaumer 2002) and unemployment stigma (Oberholzer-Gee 2000). To our knowledge no such type of study has yet been applied to program evaluation.

Our procedure differs from the correspondence testing approach in two important ways. First, we do not send matched pairs of applicants to firms. There is only one application per person to a particular job. Instead, we send applications of the *same* person with and without a basic computer course certificate to *different* jobs. This allows perfect control for individual characteristics that is missing in the studies mentioned above. While these studies put much effort in keeping the two applicants as similar as possible (except for the characteristic of interest), they are of course not completely alike. This has given rise to criticism. In his related critique of audit studies, Heckman (1998) notes, e.g., that the chances that all characteristics that might affect productivity are perfectly matched are rather low (p. 108).

The second important difference to the existing correspondence-testing studies is that our study involves no fake or deception whatsoever. In the previous studies known to us there is a varying degree of deception involved. It is, e.g., common practice to completely fake the applicants, i.e., the applicants do not exist at all. In our view this procedure is problematic for various reasons. Most importantly, there is a substantial lack of control. You can never be sure that firms detect deception (e.g., if they try to contact the applicant or a former employer). Moreover, deception makes it impossible to write complete applications. In many European countries, and in Switzerland in particular, a meaningful application includes the complete listing of jobs (with the names of former employers and their letters of recommendation) and a detailed record of education (including copies of school and diploma degrees). By using fictitious persons it is not possible to provide all this information in a legal and credible way. A further objection against deception rests on the fact that it imposes a cost on employers who take their time evaluating applications of employees who do not exist. Our study avoids deception. We had only real participants who were actually looking for jobs and who provided us with real documents and biographies.

There is a related empirical literature based on *employer surveys* that investigates differences in employer hiring by program participation status for Sweden (Agell and Lundborg, 1995 and 1999, Behrenz, 1998). A second strand of the empirical literature is concerned with employer hiring behavior and the matching of firms and workers in general, i.e., making no distinction with respect to program participation status (Albrecht and Van Ours (2002), Gorter et al. (1993), Lindeboom et al. (1994), and Van Ours and Ridder (1992)).

These studies are based on job vacancy data usually provided by the public employment service.

#### **4. Methodological advantages**

As mentioned above, the three major virtues of our study concern (i) the time proximity of the pre- and post-observations; (ii) the exogeneity of the search process; and (iii) the possibility to calculate individual treatment effects. We will now discuss these three advantages in detail.

##### **4.1 Time-proximity of pre-post observations**

As Heckman and Smith (1997) have put it, the “essence of the evaluation problem is that the same person cannot be in two or more different market states at the same time. In the training context, for each trainee, there is a hypothetical (or counterfactual) state that consists of what he or she would have done without training.” (p. 40). With the usually available data it is very difficult to construct a ‘counterfactual’ state, i.e., to find an appropriate control group for the treated one. Even with the help of sophisticated econometrics, this control is always imperfect. The method presented in this study comes closer to the ideal of constructing a ‘counterfactual’ state by collecting more appropriate data.

Our design allows us to estimate – for the *same* person – the probability to get a job interview with and without training. This pre-post comparison does not suffer from the usual problem with such comparisons that labour market conditions may differ before and after the treatment. In our design the duration between the first application (without a computer course certificate) and the last application (with the computer course certificate) was about 5 weeks and the average time difference between control and treatment observations was about 3 weeks. We managed to keep this period short because we started the application phase for the untreated application usually when individuals were close to finish the course. It is very unlikely that significant changes in the individuals’ labour market prospects take place within such short periods of time. Yet, the time span is long enough to guarantee that there were sufficiently many jobs for both the control and the treatment phase of the application process.

Furthermore, applications with and without computer course certificate are exactly the same. Firms are provided with the same CV, the same application letter and the same letters of recommendation. Thus, they have the *same* information on the characteristics that affect productivity. The only difference between the applications of the treated and the non-treated concerns the information that the applicant has been trained in a computer course. To our



knowledge, no other type of study (based on administrative data, correspondence testing or audits) guarantees this perfect individual control.

As a consequence our study does not suffer from the problem that pre-post comparisons usually are confronted with. As long as there are no systematic differences in the quality of the jobs the individual applies to with and without the certificate the outcome of applications without the computer course certificate(s) mimics the counterfactual very closely.<sup>4</sup> Note that there are several observations on job applications per individual, with and without the computer course certificate; and that the selection of the applications was under the control of the experimenter.

Besides its merits, our method does not guarantee perfect control with respect to the jobs that participants apply to. Since the same person applies with and without certificate, perfect control for job heterogeneity is impossible. The latter would imply that we send two applications of the same person to the same firm, an obviously meaningless procedure. However, since we know the job advertisements we have information on job characteristics and the firms that offer the jobs. We will control for this observable heterogeneity.

#### **4.2 Exogeneity of the search process**

The second major advantage of our set-up is that we control for *search intensity*. This control is important since program participation is time consuming and therefore search intensity may be lower (the so-called “locking-in effect of ALMP”). As a consequence, participation in a policy measure may in fact increase the duration of unemployment (Lalive, Van Ours and Zweimüller, 2001). This effect is ruled out in our design. First, the application intensity is completely exogenous to the program participant, since the researchers did the job search and wrote the application letters. Second, we *know* the frequency of applications with and without computer course certificate. Even if there was a difference in the number of applications (with and without treatment) we know the difference and can easily control for it. Therefore, we can safely rule out a possible confound which is due to differences in search activities.

Moreover, we control for *aspiration levels*. Participation in a policy measure may not only improve human capital but also aspiration levels. The first effect is obvious and is exactly the reason why the policy measure is implemented at all: People are supposed to learn and to improve their skills in a dimension that is relevant for employment. At the same time,

however, course participation might change aspiration levels and reservation wages. In particular, it might happen that people will have higher aspiration levels or higher reservation wages after completing the computer course than before starting the course.<sup>5</sup> As a consequence, it may be that previous participants do no longer apply to jobs for which they would have applied for in case of non-participation. This effect cannot be controlled for in usually available data. The present method, however, does control for this effect. On the one hand, the application process is exogenous. On the other hand, no subject refused to sign a suggested application.

#### 4.3 Individual treatment effects

One of the virtues of our research design is the possibility to calculate individual treatment effects. Heckman et al. (1999) review a number of European programs with estimated mean impacts not significantly different from zero or even negative. This does not necessarily imply, however, that there is a common negative effect on all individuals. It can also be that programs have different impacts for each individual. If this holds, it is interesting and important to know the entire distribution of the treatment effects. First, knowledge of the distribution of treatment effects is essential in addressing questions such as “For what percentage of the treated individuals is the effect positive? How many participants are doing worse because they attended the program?” These questions arise in the context of a thorough analysis of the benefits of any program. They are also important for the public support of these programs. Second, it is important to know who exactly benefits from a program in order to target existing policies to those who benefit most. Third, individuals may be reluctant to sign up for training courses due to bad prior experience with further education. Informing these individuals of the likely impacts on their employment prospects is important.

Existing data allow identification of the average effect of treatment on the treated conditional on observed characteristics such as age or gender, for instance. Our approach allows identifying the average effect of the treatment *for each individual*. Thus, it is possible

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<sup>4</sup> To make sure that the vacancies were of comparable quality before and after the treatment we sampled only vacancies that had been opened within the last week. The actual submission of the application was somewhat later, on average one week after the vacancy was opened.

<sup>5</sup> Korpi (1997) shows that ALMP participants indicate a higher subjective well-being than the openly unemployed who do not attend such a program. If subjective well-being is taken as a proxy for utility attached to the labour market status of being a program participant, this evidence suggests that program participants may be more demanding with respect to the quality of a job in the regular labour market compared to employed who are not participating in a program. This will decrease the likelihood that a job offer will be accepted by a program participant. It is in this sense that ALMPs may decrease the likelihood of accepting a regular job.

to address the question to what extent the causal effect of the program *varies* within cells defined by observable characteristics.

It is very difficult to identify individual treatment effects in existing datasets because most datasets do not contain repeated observations per individual.<sup>6</sup> To side-step this missing data problem, existing studies addressing the issue of impact heterogeneity invoke non-testable assumptions concerning the structure of the econometric model (Aakvik et al. 2000; Carneiro et al., 2002). The approach proposed in this paper identifies the effect of computer course participation for each individual because several observations with and without the computer course certificate are available. Thus, it is possible to estimate the entire distribution of the treatment impacts across individuals. This allows addressing the issue whether treatment effects are constant or whether impacts vary across the population.

## 5. Identification

This section addresses the issue under which assumptions our study identifies the causal effect of a computer course certificate on the probability of being invited to a job interview. We also highlight to what extent the present study might be superior to existing approaches in evaluating the causal effect of ALMPs on a particular outcome.

In the following we discuss identification using the model of potential outcomes due to Roy (1951) and Rubin (1974). Define by  $Y_1$  the random variable taking the value 1 if a job application *containing the computer course certificate* leads to an invitation to a job interview; let  $Y_1$  take the value 0 otherwise. We refer to  $Y_1$  as the ‘outcome with treatment’. Similarly, define by  $Y_0$  the ‘outcome without treatment’, i.e., the random variable taking the value 1 if an application *not containing the computer course certificate* leads to an invitation to a job interview. Finally, let  $D$  equal 1 if the job application actually does contain the computer course certificate, and 0 otherwise. The data is informative on  $Y = DY_1 + (1-D)Y_0$ , which corresponds to the outcome with treatment for applications containing the course certificate ( $Y = Y_1$  iff  $D = 1$ ), and the outcome without treatment for applications not containing the course certificate ( $Y = Y_0$  iff  $D = 0$ ). This paper is concerned with estimating the effect of a computer course certificate on applications that do contain such a certificate (for the individuals who agreed to participate in the present study), or

$$\Delta = E(Y_1 - Y_0 \mid D = 1) = \Pr(Y_1 = 1 \mid D = 1) - \Pr(Y_0 = 1 \mid D = 1) \quad (1)$$

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<sup>6</sup> Bergemann et al. (2000) is an exception.

This is the change in the probability of being invited to a job interview due to the computer course certificate, the so-called ‘effect of treatment on the treated’.<sup>7</sup> The fundamental problem of causal inference is that we do not observe the last quantity in the previous formula, the probability of ‘success’ in case there has been no mention of the course certificate (Holland, 1986).

However, it is possible to estimate this quantity subject to identifying assumptions. Let  $x_I$  refer to all characteristics of the individual that play a role in firms’ hiring decisions. These are, for instance, the level of education, the quality of the job application letter, previous work experience of the job seeker, etc. Let  $x_J$  denote all characteristics of the job offer that are observable to individuals who are looking for a new job such as type of skill required, size of the advertisement, working hours, industry of the hiring firm, etc. The identifying assumption underlying this study is

$$D | x_I, x_J \perp Y_0 \quad (2)$$

This so-called conditional independence assumption (CIA) asserts that, conditional on the identity of the job seeker as well as observable characteristics of the job offers, an application letter containing the computer course certificate ( $D=1$ ) must be independent of the probability of invitation ( $Y_0$ ). This assumption is justified, intuitively, if there are no unobservables that, simultaneously, determine the presence of a computer course certificate and the probability of an invitation.

CIA implies that the counterfactual outcome, the probability of invitation for treated job applications in the case there has been no computer course certificate, is identical to the corresponding probability for the job applications with no course certificate, i.e.,

$$\Pr(Y_0 = 1 | x_I, x_J, D = 1) = \Pr(Y_0 = 1 | x_I, x_J, D = 0) \quad (3)$$

Thus, the effect of the computer course certificate on the probability of invitation is identified using the probability of invitation for those applications not containing the certificate as an estimate of the counterfactual. The effect of treatment on the treated (1) is then the mean (over the characteristics  $x_I$  and  $x_J$ ) of the effect of treatment on the treated ones with particular values of  $x_I, x_J$  (3) in the population of those with treatment ( $D=1$ ).

$$\Delta = \Pr(Y_1 | D = 1) - \int \Pr(Y_0 = 1 | x_I, x_J, D = 0) dF(x_I, x_J | D = 1) \quad (4)$$

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<sup>7</sup> This effect is useful in analyzing whether a program is successful for those who participate in the program. See Heckman et al. (1999) for an definition of different treatment effects and different objectives when pursuing an evaluation study.

We can now state the methodological advantages of the present study compared to a typical cross-section study more formally. CIA is more convincing in this study for at least two reasons: *First*, because there is *no variation in characteristics of the job application* at the individual level ( $x_i$ ) it necessarily follows that there are no unobservable characteristics at the individual level that might determine, simultaneously, the decision of the researcher to include the computer course certificate in the application letter (D), as well as the decision of the firm on extending an invitation to a particular job seeker ( $Y_0$ ).<sup>8</sup> To fix ideas, let there be just one type of job offer (so  $x_j$  is constant). In that case there can be no differences in terms of match quality across treatment status. Moreover, because no characteristics of the job application letter are altered in response to treatment, there can be no effect of the quality of the job application on the probability of invitation. This implies that due to the design of this study, applications containing the information that the job seeker has undergone basic computer training must have the same probability of invitation as the corresponding applications that contain no information on course participation. Treatment status is independent of the potential ‘non-treatment outcome’ conditional on the identity of the individual job seeker. In studies that rely on cross-sectional data at the level of the job seeker, the above-mentioned conclusion does not follow. It is possible and likely that the researcher does not observe an aspect of the job application letter which is important in firms’ selection decision as well as determining the decision to either enroll into training or to disclose information on course participation. If this is the case, the CIA assumption is violated, because the outcome is correlated with the treatment status of a job application.

*Second*, in this design the allocation of job offers to the treatment was entirely under the control of the researcher, as pointed out in the previous section. Thus, the assumption that there are no remaining unobservables at the level of the job application that determine the treatment status of an application as well as the probability of an invitation is, arguably, justified. Recall that the data contains information on all characteristics of a job offer that can potentially be observed (in the newspaper or in the Internet) and that are relevant in selecting job offers.

Existing evaluation studies based on cross-section data at the level of the individual contain only little if any information at the level of the vacancy. This implies that it is not possible to condition on the type of vacancy in the evaluation. The consequence is that it is

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<sup>8</sup> Recall that all aspects of the job application letter were held constant across treatment status.

not possible to discuss the causal effect of training on the probability of invitation to a job interview.<sup>9</sup>

CIA is, arguably, valid in this application because the treatment status was decided upon by the researcher, because there was no variation in observable characteristics conditional on the identity of the job seeker, and because the data is informative on all characteristics which were relevant in assigning job applications to treatment status.

## 6. Results

In this section we describe the dataset, report estimates referring to the effect of basic computer course certificates on the chances of getting a job interview and investigate the heterogeneity in the effect of computer programs along two dimensions: skills required on the new job and individual background of the job seeker.

### 6.1 Data

The study was conducted from December 1999 until March 2000. In total, 10 individuals participated in the study. A total of 191 job applications was sent out, 95 without computer course certificate, 96 with computer course certificate. On average, we sent out the treated application 21 days after sending out the control application. Thus, in principle both applications could have been sent to the same position. It was ensured that the time span between the date of publishing and the date of sending out the application was not more than one week.

The data contains detailed information on the type of job offer. The first type of information refers to skills required on the vacancy: computer skills, knowledge of a second language and ability to work in a team. These three variables were coded as dummy variables, taking the value 1 if the job offer mentions the item and 0 otherwise. Second, a distinction was made between white-collar jobs and blue collar jobs. Third, there is information on the industry of the firm seeking to hire (government, retail, and other services are the largest three industries), hours of work (as a share of a full time job), the size of the job advertisement in the newspaper (in cm<sup>2</sup>), and the date when the vacancy was published. The last characteristic

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<sup>9</sup> Instead, such studies identify the causal effect of training on invitations *and* on the process of finding suitable job offer. Of course, this effect may be of interest. However, in discussing the effects of skill enhancement on labour market success, this combined effect is not informative. If it is the case that training improves the process of finding job offers instead of enhancing computer skills, one might think about sending job seekers to programs aiming at improving job search skills instead of computer skills.

is important because all treated applications were sent out later than the control applications. These characteristics are the set of observables  $x_i$  used in this study.

With respect to individuals, the data are informative on education, age, sex, previous work history, quality of the application, etc. As the data contain repeated observations for each individual, both in the treated and in the non-treated state, it is possible to account for such individual differences by way of person effects.

The dependent variable is whether an application letter leads to an invitation within three weeks after sending out the job application. We find that 83 % of the firms responded within this time period - evidence that the response period was chosen sufficiently long. Note that the fraction of missing responses was identical across treatment status. The variable “invitations to a job interview” is coded as a dummy taking the value 1 if the hiring firm invites the job seeker, and 0 otherwise. Note that the 0-outcome refers to the response that the firm does not wish an interview (76 % of all zero entries), the firm wants further information (4 %), and no response on the part of the hiring firm (20 %).

## **6.2. Effect of Treatment on the Treated**

The choice of the appropriate estimation strategy was guided by the trade-off between bias and variance. Non-parametric estimators, such as the method of matching, have the advantage – compared to parametric approaches – that no assumptions beyond CIA stated in (2) need to be invoked. This implies that the bias due to misspecification of the econometric model is reduced. However, it is well known that non-parametric estimators are biased in small samples. Moreover, non-parametric estimators are characterized by a slow speed of convergence to the asymptotic distribution. Considering the fact that the number of job applications in this study is small, we report estimates based on a parametric estimator.<sup>10</sup>

The dependent variable, invitations to a job interview, is a binary variable. This variable can be analysed using either a logit or a probit model. However, these maximum-likelihood based methods require knowledge about the distribution of the error term, and rely on large samples as far as the consistency and asymptotic distribution is concerned. A potentially attractive alternative to the logit / probit approach is the linear probability model. This model assumes that the probability of an invitation to a job interview can be approximated by a linear function of the explanatory variables. The use of the linear probability model has been justified in the literature in case the focus of research is the estimation of the effect of an

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<sup>10</sup> See Heckman et al. (1998) for a discussion of the matching estimator.

explanatory variable on the change in the probability of a non-zero realisation of the dependent variable (Moffit, 1999).

The main advantage of the linear probability model is that there is no need to assume a specific functional form for the error term. Objections against the linear probability model include that it may produce fitted values outside the zero-one range, and that errors are heteroskedastic.<sup>11</sup> The first concern cannot be accounted for. Nevertheless, it is true that the linear probability model performs quite well near the sample average of the dependent variable. Heteroskedastic errors can be addressed in different ways. We report bootstrap standard errors that are robust to the presence of heteroskedastic errors.<sup>12</sup> Note that a popular alternative approach to address heteroskedasticity involves the White (1980) estimator of variance. This method is likely to lead to unsatisfactory results since this method is valid only for large samples. Table A1 in Appendix 1 contains complete results with White standard errors.

Table 1 reports results regarding the effect of a computer course certificate on the probability of invitation to a job interview based on a linear probability model. All estimates control for individual effects and the observed characteristics of the vacancy mentioned in the previous subsection. Column A reports an estimate of the effect of treatment on the treated based on the assumption that the effect of the course certificate neither differs by individual nor by type of job offer.

According to the estimate in Column A, the effect of the computer course certificate on the success of job applications was negative but not significantly different from zero. The point estimate suggests a reduction of the probability of being invited for a job interview by 3.3 % if a computer course certificate is added to the application. However, the result in Column A may be biased because the underlying assumption is that the treatment effect is constant across individuals and jobs. Therefore, it is important to relax this assumption.

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<sup>11</sup> Aldrich and Nelson (1984) analyze the properties of the linear probability model in detail.

<sup>12</sup> We perform a bootstrap on the sample observations. This method has been shown to be robust to heteroskedasticity and it is the appropriate method to use in the presence of random explanatory variables. Clearly, these conditions are both fulfilled in the present application. See Maddala and Jeong (1993) and Efron (1993) for a discussion of bootstrap methods and their applications.



**Table 1. The effect of a computer course certificate on invitations to a job interview**  
**Dependent variable: Probability of being invited to a job interview**

	A	B	C	D
Average effect of treatment on the treated	-.033 (.061)			-.052 (.094)
Average effect of treatment for vacancies that				
... do not mention computer skills		.043 (.068)		
... do mention computer skills <sup>a)</sup>		-.195 (.095)**		
Average effect of treatment for				
Individual 1			-0.178 (0.122)	
Individual 9			-0.157 (0.250)	
Individual 5			-0.131 (0.125)	
Individual 4			-0.122 (0.123)	
Individual 7			-0.057 (0.077)	
Individual 6			-0.056 (0.082)	
Individual 3			-0.051 (0.234)	
Individual 2			0.011 (0.093)	
Individual 10			0.198 (0.171)	
Individual 8			0.233 (0.224)	
Observations	191	191	191	191
Adjusted R-squared	0.25	0.27	0.25	0.24

Notes: Bootstrap standard error in parentheses (based on 1000 repetitions), see Table A1 for White standard errors (which are not valid in small samples).

\*, \*\*, \*\*\* denote significance at the 10%, 5%, and 1% level.

See Table A1 in Appendix 1 for results on other variables (computer skills, second language, team skills, date vacancy was published, size, work hours, skill level, position, industry, and 10 individual effects).

A: Assumes constant treatment effect.

B: Allows for heterogeneity of the treatment effect with respect to “computer skills” only.

C: Allows for heterogeneity of the treatment effect with respect to individuals.

D: Allows for full heterogeneity of the treatment effect with respect to all variables.

a) Measured relative to “job does not mention computer literacy”.

Source: Own calculation, own survey data.

### 6.3. Effect heterogeneity: ‘Computer jobs’ and individual heterogeneity

Columns B and C in Table 1 report results allowing for differences in the estimated effect of computer courses along two important dimensions. First, we allow for separate treatment effects for vacancies that do or do not require ‘computer skills’ (column B). Second, we allow for different treatment effects at the individual level (column C)

The estimates in column B suggest that the effect of computer training on jobs that do not require computer skills is slightly positive but insignificant. Interestingly the effect for jobs that do require computer skills is significantly lower than the effect for jobs that do not require computer skills: The ‘invitation probability’ drops by 19.5 % if job seekers disclose information on the recent completion of a basic computer.

While this result appears puzzling at first, a potential explanation may focus on the fact that certificates actually confer two separate and opposing types of information. The first type of information refers to the new skill acquired and is likely to increase invitations. The second type of information is that the job seeker does not have prior work experience with the computer and that therefore the level of computer education is in fact rather low. This information may have a negative impact on the success of job applications. It is plausible that for some jobs, the first, positive effect dominates whereas for other types of jobs the second, negative effect dominates. The negative effect will, for instance, dominate in the case of vacancies stating computer skills as a requirement to do the job.<sup>13</sup>

Column C explores differences in the effect of computer courses along the individual dimension. Specifically, we report separate effects of computer training programs for each individual. This is possible because the data contain repeated information on invitations for each individual in each treatment state. Results suggest that there is tremendous variation in the point estimates of the individual treatment effects. The estimated effects of computer training on the chances of getting a job interview range from a reduction by 17.8 % (Individual 1) to an improvement by 23.3 % (Individual 8), covering a range of 41.1 %.<sup>14</sup> These results suggest that targeting of the treatment is critical. In a next step, it may be useful to relate these individual treatment effects to further information at the individual level.

It is interesting to see whether computer training did benefit those who already had a rather high probability of being invited without such training. Figure 1 displays the scatter

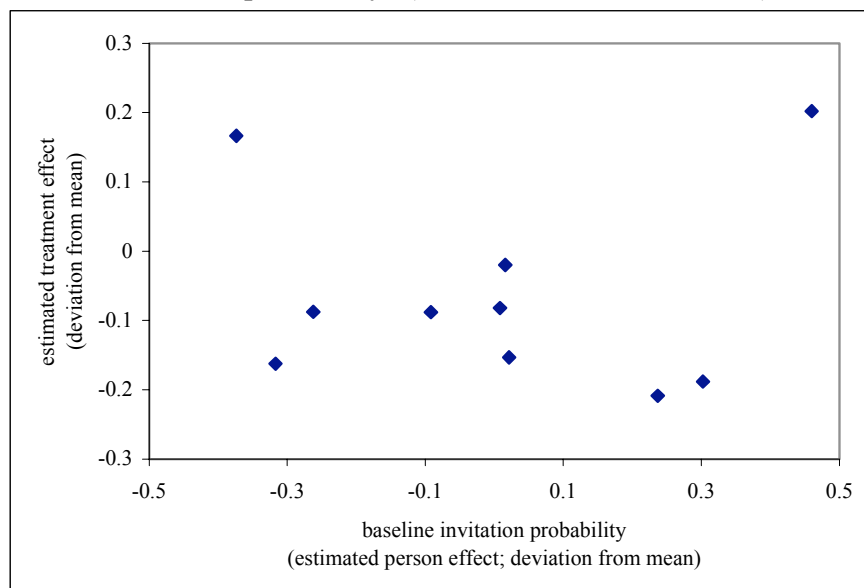
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<sup>13</sup> This finding is consistent with job market signaling theories (Spence, 1973) and recent evidence on the role of formal education in job market signaling (Albrecht and Van Ours, 2002).

<sup>14</sup> This difference is at the border of statistical significance at the 10 % level with a bootstrap standard error of 25.8 %.

plot of the estimated treatment effect against the baseline probability of being invited to a job interview across individuals.<sup>15</sup> Figure 1 suggests that basic computer training is the least ineffective for the individuals who are below the average probability of invitation.<sup>16</sup> Again, this is consistent with a signalling explanation. The computer course certificate confers the least negative information for those individuals with an a priori weak labour market position.

**Figure 1. Does computer training benefit those job seekers with a high baseline invitation probability? (based on Table 1 Column C)**



Both sensitivity analyses document that heterogeneity in the effect of computer training courses on invitations to a job interview is important. Column D in Table 1 therefore reports the estimated average effect of treatment on the treated based on a linear probability model allowing for heterogeneity in the treatment effect along *all observed dimensions*. This means that the treatment indicator is interacted with all observed characteristics of the vacancy as well as with the 10 person effects. The effect displayed in the table is the product of these separate treatment effects with the average of the characteristic in the sample with treatment. This is an estimator of the population effect of treatment on the treated defined in (4) (see Appendix 2 for an exact description of the procedure used).

<sup>15</sup> Both estimates are deviations from the respective sample means (-3.1 % for the treatment effect, and 26.3 % for the person effect).

<sup>16</sup> The correlation between the estimated treatment effect and the estimated person effect is 0.01 with and -0.61 without individual 8 (the ‘outlier’ in the upper right corner of Figure 1).

The results suggest that the effect of training on labour market success is negative but insignificant. The estimated average effect of treatment on the treated individuals is slightly smaller than the effect reported in Column A, but the difference is neither statistically significant nor quantitatively important.<sup>17</sup> This means that, in the present context, heterogeneity in the treatment effect does not bias results based on a model assuming homogeneous treatment effects.

## 7. Conclusion

The main difficulty in the evaluation of social programs is to obtain credible estimates for the ‘counterfactual’, i.e., information for participants on the outcome in the state of non-participation. A meaningful indicator for the success of active labour market policies is the probability of being invited to a job interview. This measure can be observed both during and after the program for participants. Thus, the effect of the program on invitations is identified under weak assumptions *for every individual*.

The present study has shown that this data collection procedure is operational. Moreover, we find that this approach is useful when discussing heterogeneous treatment impacts. If program impacts are heterogeneous, it is important to find out who benefits most from the program in order to target the program. The approach suggested in the paper provides the information needed in order to target the program.

We find that the average effect of a computer training program in Switzerland on invitations to a job interview is negative but insignificant. Looking at individual treatment effects we see quite variance in outcomes: there are both individuals who benefit and those who do actually worse after program participation. An improvement in the targeting of the program seems important.

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<sup>17</sup> Note, that allowing for heterogeneous treatment effects does not improve the fit of the model as indicated by the reduction in the adjusted  $R^2$  from .25 to .24.

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## Appendix 1

**Table A1. Detailed results for Table 1**

Dependent variable: Invitations to a job interview

	A	B	C	D
Treated	-0.033 (0.047)	0.043 (0.061)		
Treated * pc skills		-0.195 (0.093)**		-0.141 (0.113)
Treated * pd1 <sup>a)</sup>			-0.178 (0.121)	-0.361 (0.360)
Treated * pd2			0.011 (0.083)	-0.113 (0.344)
Treated * pd3			-0.051 (0.200)	0.100 (0.349)
Treated * pd4			-0.122 (0.066)*	0.103 (0.338)
Treated * pd5			-0.131 (0.066)**	0.429 (0.445)
Treated * pd6			-0.056 (0.051)	0.372 (0.322)
Treated * pd7			-0.057 (0.058)	0.376 (0.402)
Treated * pd8			0.233 (0.200)	0.500 (0.419)
Treated * pd9			-0.157 (0.219)	0.082 (0.378)
Treated * pd10			0.198 (0.153)	0.541 (0.361)
Treated * language skills				0.146 (0.142)
Treated * team skills				0.028 (0.122)
Treated * Date of publication <sup>b)</sup> (weeks since mean publication date)				-0.040 (0.031)
Treated * size of vacancy in newspaper (cm <sup>2</sup> / 100)				-0.104 (0.073)
Treated * hours (share of full time)				0.230 (0.504)
Treated * white collar position <sup>c)</sup>				-0.018 (0.235)
Treated * government				-0.208 (0.182)
Treated * retail				-0.210 (0.190)
Treated * other services				-0.025 (0.196)
Computer skills	-0.082 (0.050)	0.013 (0.069)	-0.100 (0.054)*	-0.039 (0.075)
Language skills	0.112 (0.071)	0.093 (0.069)	0.111 (0.074)	0.015 (0.107)
Team skills	0.076 (0.054)	0.081 (0.054)	0.095 (0.057)	0.093 (0.101)

(Continued on next page)



Table A1 (continued)

Date of publication (weeks since mean publication date)	0.009 (0.003)***	0.009 (0.003)***	0.010 (0.003)***	0.010 (0.003)***
Size of vacancy in newspaper (cm <sup>2</sup> / 100)	-0.025 (0.031)	-0.023 (0.032)	-0.040 (0.037)	0.011 (0.054)
Hours (share of full time)	-0.123 (0.179)	-0.159 (0.182)	-0.187 (0.184)	-0.350 (0.467)
White collar position	-0.204 (0.098)**	-0.194 (0.100)*	-0.233 (0.112)**	-0.227 (0.170)
Government	-0.009 (0.098)	-0.020 (0.098)	-0.011 (0.107)	0.109 (0.129)
Retail	0.042 (0.100)	0.022 (0.101)	0.034 (0.105)	0.141 (0.133)
Other Services	0.051 (0.101)	0.057 (0.101)	0.081 (0.111)	0.108 (0.139)
Pd2 <sup>d)</sup>	-0.106 (0.061)*	-0.123 (0.062)*	-0.221 (0.112)**	-0.240 (0.122)*
Pd3	-0.140 (0.087)	-0.155 (0.089)*	-0.229 (0.149)	-0.243 (0.172)
Pd4	-0.203 (0.097)**	-0.209 (0.100)**	-0.215 (0.121)*	-0.264 (0.137)*
Pd5	-0.488 (0.125)***	-0.488 (0.126)***	-0.554 (0.161)***	-0.595 (0.220)***
Pd6	-0.421 (0.119)***	-0.396 (0.122)***	-0.499 (0.164)***	-0.600 (0.236)**
Pd7	-0.266 (0.095)***	-0.270 (0.095)***	-0.329 (0.137)**	-0.453 (0.191)**
Pd8	0.367 (0.146)**	0.393 (0.146)***	0.223 (0.199)	0.182 (0.222)
Pd9	0.111 (0.121)	0.086 (0.119)	0.065 (0.175)	-0.029 (0.210)
Pd10	-0.380 (0.141)***	-0.360 (0.142)**	-0.610 (0.185)***	-0.600 (0.255)**
Constant	0.392 (0.158)**	0.358 (0.158)**	0.500 (0.187)***	0.511 (0.266)*
Observations	191	191	191	191
Adjusted R-squared	0.25	0.27	0.25	0.24

White standard errors in parentheses. Table 1 in the main text reports bootstrap standard errors.

\*\*\* is significant at 10%; \*\* significant at 5%; \* significant at 1%.

a) Pd1: Dummy variable taking the value 1 if the observation refers to Individual 1.

b) 'Other industries' is reference.

c) Size, date published, hours are deviations from the sample mean.

d) Note that the person dummies measure the difference of the invitation probability of this person to person 1 because this dummy was left out (for ease of interpretation).

## Appendix 2

This section describes the two step procedure used to estimate the average effect of treatment on the treated, defined in (1), allowing for separate effects on all observable dimensions (Table 1, Column D). The first step is a simple regression of the invitation indicator  $y_j$  on (i) interactions of the treatment indicator  $D_j$  with all observables of the job offer  $x_j$  and with all person dummies  $a_j$ , and (ii) on the  $x_j$  and  $a_j$  directly. The notation  $a_j$  refers to the person dummy of the individual to whom application  $j$  belongs, and  $j$  indexes job offers. The regression thus reads

$$y_j = \sum_k \delta_{x,k} x_{k,j} D_j + \sum_l \delta_{a,l} a_{l,j} D_j + \sum_k \beta_{x,k} x_{k,j} + \sum_l \beta_{a,l} a_{l,j} + \varepsilon_j$$

where  $k$  indexes the different observable characteristics (such as computer skills, or hours of work, etc.) and  $l$  indexes the individuals, and runs, therefore, from 1 to 10.

In the second step, the average effect of treatment on the treated is estimated. Recall that this effect is defined in the population as

$$\Delta = E(Y_1 - Y_0 \mid D = 1) \quad (\text{A1})$$

An estimate of  $E(Y_1 | x_j, a_j, D=1)$  is

$$\hat{y}_{1,j} = \sum_k \hat{\delta}_{x,k} x_{k,j} + \sum_l \hat{\delta}_{a,l} a_{l,j} + \sum_k \hat{\beta}_{x,k} x_{k,j} + \sum_l \hat{\beta}_{a,l} a_{l,j} \quad j \in \{j : D_j = 1\}$$

where  $\hat{\cdot}$  denotes the OLS estimator. If CIA holds,  $E(Y_0 | x_j, a_j, D=1)$  can be estimated by

$$\hat{y}_{0,j} = \sum_k \hat{\beta}_{x,k} x_{k,j} + \sum_l \hat{\beta}_{a,l} a_{l,j} \quad j \in \{j : D_j = 1\}$$

It follows that the average effect of treatment on the treated in (A1) is

$$\begin{aligned} \hat{\Delta} &= \frac{\sum_j I(D_j = 1) (\hat{y}_{1,j} - \hat{y}_{0,j})}{\sum_j I(D_j = 1)} = \frac{\sum_j I(D_j = 1) (\sum_k \hat{\delta}_{x,k} x_{k,j} + \sum_l \hat{\delta}_{a,l} a_{l,j})}{\sum_j I(D_j = 1)} \\ &= \sum_k \hat{\delta}_{x,k} \bar{x}_k + \sum_l \hat{\delta}_{a,l} \bar{a}_l \end{aligned} \quad (\text{A2})$$

where  $I(\cdot)$  is the indicator function, and the over bar on the second line in (A2) indicates “mean for applications with treatment”. In words, the estimated average effect of treatment on the treated is the product of the estimated treatment coefficients with the mean of the characteristics of the application.

The standard error for this quantity was estimated by bootstrapping according to the following procedure

1. select at random, with replacement, 191 observations on  $y_j$ ,  $x_j$ , and  $a_j$ ,
2. calculate the quantity in (A1)
3. repeat this process 1000 times.

the standard deviation of the resulting 1000 estimates of the treatment effect ( $\hat{\Delta}_i$ ) was calculated according to the formula

$$sd(\hat{\Delta}) = \sqrt{\frac{\sum_{i=1}^{1000} (\hat{\Delta}_i - \text{mean}(\hat{\Delta}_i))^2}{1000 - 1}}$$

This type of bootstrap allows for heteroskedasticity (Maddala and Jeong, 1993).

### **Appendix 3**

#### **Application letter WITHOUT course certificate (=control application; translated from German)**

<Address of individual>

<Address of firm>

<Date>

**< job title >**

Dear <Personnel department>

In <newspaper> of <date> you were looking for a new employee with < skill 1> and < skill 2> and < skill 3> for <job title>. Because I am well prepared to fill this position I would like to apply for this job.

I recently <description of recent past>

My skills are <list of skills>

Looking forward to your response.

Yours Sincerely

<Signature>

Enclosed: CV, Letters of recommendation

**Anonymous application letter WITH course certificate  
(=treated application; translated from German)**

<Address of individual>

<Address of firm>

<Date>

**< Job title >**

Dear <Personnel department>

In <newspaper> of <date> you were looking for a new employee with < skill 1> and < skill 2> and < skill 3> for <job title>. Because I am well prepared to fill this position I would like to apply for this job.

I recently <description of recent past>

My skills are <list of skills>

I recently finished the course "Textverarbeitung (WinWORD)" at EB Wolfbach, Zurich.  
The enclosed course certificate indicates successful completion of this course.

Looking forward to your response.

Yours Sincerely

<Signature>

Enclosed: CV, Letters of recommendation, Course certificate

## Course certificate

**Erwachsenenbildung**

**Kursbestätigung**

Berufsschule für Weiterbildung Zürich  
IA 20.11

**E · B**  
wolfbach

Berufsschule für Weiterbildung Zürich  
Kantonsschulstrasse 3

**Jürg Heller**  
hat den folgenden Kurs

**Textverarbeitung (WinWORD)**  
Dauer: 9 Halbtage  
mit Erfolg besucht

**Kursinhalte**

- Texte erfassen, speichern und ausdrucken
- Korrekturen ausführen
- Gestalten von Texten

**Kursleitung**  
Teresa Besenfelder

**E · B**  
wolfbach

  
Karl Grob  
Schulleitung  
EB Wolfbach

Zürich, 7. Juli 1999  
IA 20.11

(Note that “Jürg Heller” does not exist in real life)